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THE REFINED STRUCTURE OF THEORIES*

Introduction

This contribution gives a systematic introduction to the structuralist reconstruction of empirical theories, which is presupposed in most of the other contributions. It starts from a number of global ideas about the nature and structure of empirical theories. To begin with, it is frequently useful to distinguish a core of principles and a belt of auxiliary hypotheses.

A theory is said to be ontologically stratified when there are two or more kinds of entities involved. In this case the principles usually concern only one kind of entities and their properties, and may be called internal principles, whereas the auxiliary hypotheses connect the different kinds of entities and their properties, and may be called bridge principles. It may or may not be plausible to speak of a lower, micro-level and a higher, macro-level.

Besides ontological stratification there is epistemological stratification: they frequently go together, but are essentially independent.

A proper theory *T* can be defined as an epistemologically stratified theory in the sense that it contains terms, and hence statements, that are laden with one or more of its principles, that are *T*-laden or *T*-theoretical, for short. The other terms of *T* are called *T*-unladen or *T*-non-theoretical. In contrast to proper theories, experimental hypotheses can be defined as improper theories, containing no theoretical terms of their own. A set of

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* This text is an adapted version of a chapter of an advanced textbook in philosophy of science in preparation, provisionally entitled *Cognitive-heuristic patterns in science*. Brief presentations of the examples of classical particle mechanics, of the periodic table of chemical elements, and of the psycho-analytic theory have been deleted, as well as a section on relations between theories and theory-nets. Instead of the latter, a section on idealization and concretization has been inserted.

connected experimental hypotheses is called an experimental theory. It should be noted that being *T*-non-theoretical is a theory-relative qualification of a term or a statement: they may well be laden with underlying theories.

The main function of a proper theory *T* is the explanation and prediction of *T*-unladen, experimental laws, i.e., true general hypotheses containing no terms laden with themselves or *T*. For this function the distinction between experimental and proper theories is of course crucial.

A last general feature of theories to be mentioned beforehand is that the principles of a theory, whether ontologically and/or epistemologically stratified or not, can frequently be subdivided into main principles, claimed to be true for the whole domain concerned, and special principles, only claimed to be true for a certain subdomain.

Why the Structuralist Approach?

There are two main approaches to the fine-structure of empirical theories. The *statement*-approach conceives theories primarily as sets of statements. In the case of an axiomatized theory all these statements are logical consequences of a subset of the axioms. This approach has long been considered as the only and obvious approach, e.g. by Carnap and Popper.

The set-theoretic or *structuralist* approach was introduced by Suppes and refined by Balzer, Sneed, and Stegmüller. Its basic idea is that theories frequently specify classes of set-theoretic structures satisfying certain conditions. A set-theoretic structure is an ordered set of one or more domain- or base-sets and one or more properties, relations or functions defined on them, which satisfy certain conditions.

For example a biological family is a structure $<A,C>$ with A as the (base-)set of members of the family and C as a ternary relation on A, i.e., C is a subset of $A \times A \times A$, such that $C(x,y,z)$ indicates that z is a child of x and y. It is a proper two-generation biological family if there are precisely two members x and y in A such that for all other z in A it holds that $C(x,y,z)$.

According to the structuralist view an axiomatized theory defines such a class of structures and the conditions imposed on the components of the structures are the axioms of the theory. The link with reality is made by the claim, associated with the theory, that the set of set-theoretic representations of the so-called intended applications form a subset of the class of structures of the theory.

Unfortunately, there has been much disagreement about what the proper approach to theories is, whereas it is easy to see that the two approaches are not at all incompatible. At least for so-called first-order statement theories, i.e., theories formulated as a set of statements of a so-called first-order language, it is evident that the set of models of the theory, i.e., the structures for which the statements of the theory are true, is precisely a set of structures that might also have been introduced directly in the structuralist way. If we do not restrict ourselves to first-order languages both approaches are essentially intertranslatable. Hence, the choice is a pragmatic question.

The main advantage of the structuralist approach is that it is much more a bottom-up approach than the statement approach. It invites one as it were to represent and analyse theories and their relations as closely to the actual presentations in textbooks as formally possible. As in scientific practice all kinds of useful mathematics may be used for that purpose. It does not mean that structuralist reconstruction of theories is an easy task. However, the statement approach is certainly more difficult for specific reconstructions. It is primarily useful in talking about theories in general and in studying logical, in particular model-theoretic, questions about the relation between sentences and their models. These questions and their answers become very complicated as soon as substantial mathematics is involved, e.g. real numbers. However, as we will indicate, even theoretical questions concerning for instance idealization and concretization as a truth approximation strategy can be treated relatively easily in structuralist terms.

Given our intention to be as useful as possible for actual scientific research I will restrict the attention to the structuralist approach. The best textbooks presenting the structuralist view on theories are Balzer [1982], Diederich [1981], Stegmüller [1973/1986] and Balzer, Moulines, Sneed [1987]. I will present the main general aspects without going into technical details which are not of primary importance for actual practice. My main goal is to make clear what kind of entities one may be looking for in theory formation and how standard questions about these entities can be explicated. Moreover, in passing I will explicate some standard Popperian concepts in structuralist terms.

The Slide Balance

Consider the slide-balance of *Figure 1*. On either side there can be placed any finite number of objects of various weights at all possible distances from the fulcrum S. The balance is assumed to be completely symmetric, the equal arms are as long as necessary, and the objects are pointmasses, i.e., dimensionless particles. Our domain of interest is the equilibrium states, i.e., all possible distributions of objects leading to equilibrium.

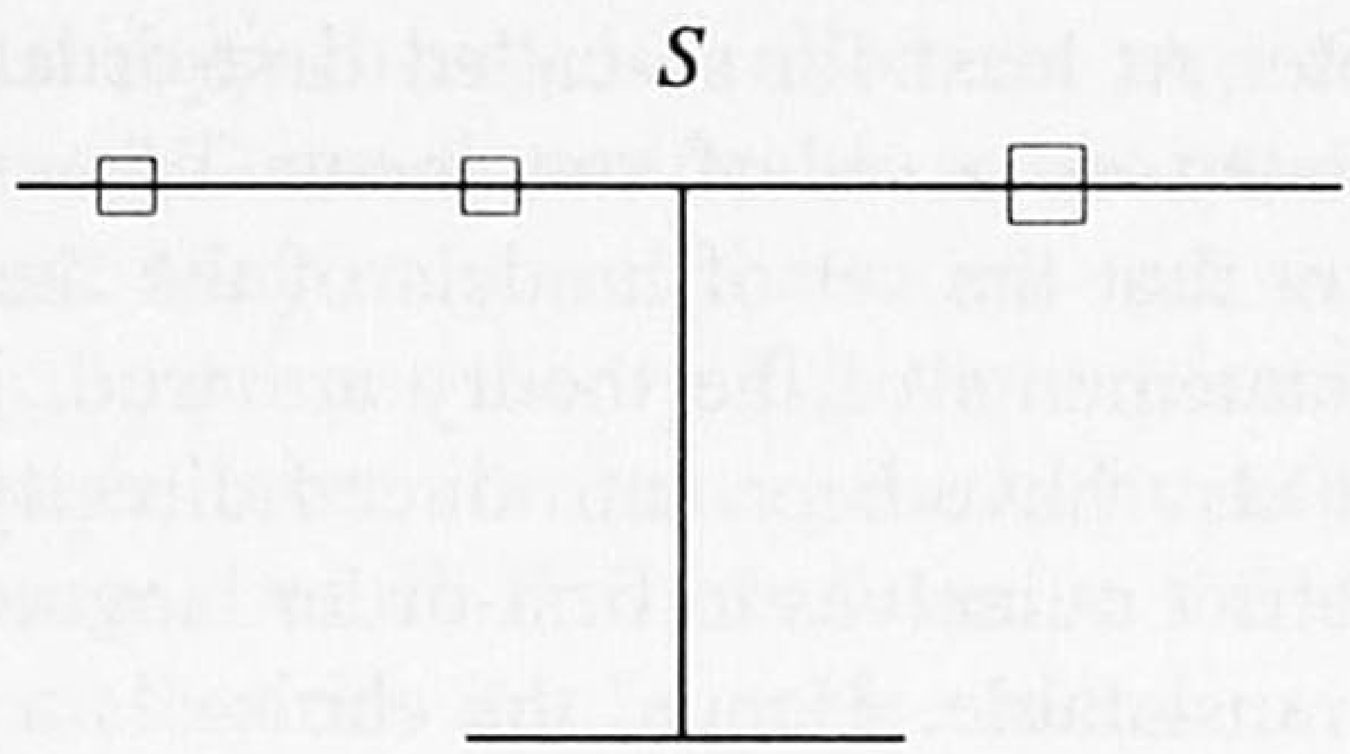


Figure 1

A plausible way to represent the equilibrium states, the intended applications, is as follows. We start by characterizing a possible or potential equilibrium state by a structure of the form $<P,Pl,d,w>$, where P is the finite set of particles involved and Pl the subset of P such that Pl and P-Pl represent the particles to the left and to the right of S, respectively. For every particle p in P, $d(p)$ indicates the distance of p from S and $w(p)$ the weight of p. Technically speaking, d and w are positive real-valued functions on P. Let us call the set of structures $<P,Pl,d,w>$ satisfying all formal conditions the set of potential equilibrium models of our theory about the equilibrium states of the slide-balance, indicated by *SBp*.

Accordingly, our conceptual claim is that the equilibrium states can be represented as members of *SBp*, i.e., there is a subset E of *SBp* representing the real, that is, the empirically possible, equilibrium states: the *SBp*-set of intended applications.

The ultimate purpose of theory formation now is to try to characterize E explicitly by one or more additional conditions. As is well known, the adequate condition in the present case is specified by the so-called law of the balance: the sum of distance times weight of the objects on the left should be equal to the corresponding sum with respect to the objects on the right. Let us call the subset of *SBp* of members satisfying this condition the set of equilibrium models of our theory, indicated by *SB*. The proper empirical claim of the law of the balance can now be formulated as "$E = SB$", and this claim is true, *ceteris paribus*.

As we will see in other cases the relevant claim need not be as strong as in the present case. It might just have been the claim that E is a subset of *SB*. It will be helpful for later examples to add a more formal presentation of the naive theory of the slide-balance ($\mathbb{R}^+$ indicates the set of positive real numbers).

The naive theory of the slide-balance $< SBp, SB, D, E >$

	$\longrightarrow$ contains $< P, Pl, d, w >$	iff
	1) P is a finite set	(particles)
	and Pl a subset	(the particles on the left of S)
	2) d: $P \Rightarrow \mathbb{R}^+$	($d(p)$: the distance of p from S)
SBp	3) w: $P \Rightarrow \mathbb{R}^+$	($w(p)$: the weight of p)
	4) *the law of the balance*	
SB	$\Sigma p \in Pl\ d(p) \cdot w(p) = \Sigma p \in P\text{-}Pl\ d(p) \cdot w(p)$	
SBp-SB	*empirical content* (to be explained)	
$E \subseteq SBp$	*conceptual claim*: the intended domain of applications can be represented as potential models, the intended applications	
$E \subseteq SB$	*(naive weak) empirical claim*: the intended applications are equilibrium models (naive strong claim: $E = SB$)	

Later we will see that the empirical claims are naive, because it appears to be impossible to test them in a non-circular way. But first I will present the general structuralistic set-up for unstratified theories.

Unstratified Theories

Let there be given a domain D of natural phenomena (states, situations, systems) to be investigated. D is supposed to be circumscribed by some informal, intensional description and may be called the *intended domain of applications*. Although D is a set, its elements are not yet mutually well distinguished. For this reason we do not yet speak of domain of intended applications.

In order to characterize the phenomena of D a set Mp of *conceptual possibilities or potential models* is construed. Mp is, technically speaking, a set of structures of a certain type, a so-called similarity type. In practice Mp will be the conceptual frame of a research program for D.

The confrontation of D with Mp, i.e., D seen through Mp, is assumed to generate a unique, time-independent subset $Mp(D) = I$ of all Mp-representations of the members of D, to be called the Mp-set of *intended applications*. Apart from time-independence, this is a conceptual claim. Of course, I will be a subset of the (Mp-)set of *empirical possibilities*, but it may be a proper subset, i.e., a more specific set of intended applications satisfying certain additional (more or less precise, but relatively observational) conditions.

Assuming that the set of empirical possibilities is a proper subset of *Mp*, i.e., not everything which is conceivable is empirically possible, *I* is also a proper subset of *Mp*. In certain cases *I* may be a one-element set, in particular when we want to describe the 'the actual world' in a certain context, that is, a realized empirical possibility, e.g. the description of conditions and results of a particular experiment.

A specific theory about *D* centers around an explicitly defined subset *M* of *Mp*, the *models* of the theory. More specifically, a specific unstratified theory is any combination of the form $UT = \langle Mp, M, D, I \rangle$ with, beside the conceptual claims that *M* and *I* are both subsets of *Mp*, the (weak) empirical claim that *I* is a subset of *M*. Sometimes the strong empirical claim is made that *I* is equal to *M*, but we take the weak claim as standard. It is plausible to call *UT* true when its claim is true and false otherwise.

The general set-up of the structure of epistemologically unstratified theories will now be presented in a scheme. Such a theory is a meta-structure of the following form:

$\langle Mp, M, D, I \rangle$ is an *epistemologically unstratified theory* iff

Mp:	potential models: a set of structures of a certain type
$M \subseteq Mp$:	models: the potential models that satisfy all axioms
Mp-M:	empirical content (to be explained)
D:	the intended domain of applications
$I \subseteq Mp$:	intended applications: conceptual claim: *Mp*-representation of *D* leads to the subset *I* of *Mp*
$I \subseteq M$:	empirical claim (strong claim: $I = M$)

$\langle Mp, M \rangle$ is sometimes called the *theoretical core* of the theory, and $\langle D, I \rangle$ may be called the *application target* of the theory.

The unstratified set-up of theories seems to be rather adequate for experimental theories, a combination of one or more experimental hypotheses, which contain by definition only terms that are understood independently of the theory concerned.

Basic Terminology

Before we go over to stratified theories, I we like to present some useful basic terminology, which can largely be seen as a structuralist explication of Popperian 'statement terminology' [Popper, 1959]. I will neglect all necessary provisos, in particular in regard to the complications arising from underlying theories. To use Lakatos's term [Lakatos, 1978], I explicate naive falsificationism, first unstratified, later stratified.

When the claim of theory $UT = <Mp,M,D,I>$ is false I-M is by definition non-empty, in which case it is plausible to call its members instantial mistakes or (empirical) *counter-examples* of UT. Note that being a counter-example in this sense does not imply that it has been realized already and registered as such. The set of counter-examples I-M is by definition a subset of Mp-M. Hence, I-M can, whatever I is, only be non-empty when Mp-M is non-empty. In other words, the members of Mp-M may be called the *potential* counter-examples of the theory and, as was already announced a number of times, the set Mp-M itself the *empirical content* of UT. From the present point of view Popper had this in mind with his notions 'potential falsifier' and 'empirical content', respectively.

Other plausible explications of Popperian terminology are easily found. UT is *falsifiable* (or empirical) if and only if Mp-M is non-empty. UT^* is *better falsifiable* than UT when Mp-M is a proper subset of Mp-M^*. The latter condition is equivalent to: M^* is a proper subset of M. In its turn, this is equivalent to stating that the claim of UT^* implies that of UT, and not conversely, that is, UT^* is stronger than UT.

The verification/falsification asymmetry also arises naturally in the present set-up. To *verify* theory UT it would be necessary to show that all members of I, that is all Mp-representations of D, belong to M. In interesting cases this will always be an infinite task, even in the case that I is finite, for the task is only finite when D is finite. To *falsify* UT it is 'only' necessary to show that there is *at least one* member of I not belonging to M. Hence, if a theory is true, verification will nevertheless not be obtainable if D is infinite. On the other hand, when a theory is false, falsification is attainable in principle, viz. by realizing one counter-example. Of course, if an attempt to falsify fails in such a way that the experiment provides an (*empirical*) *example* of UT, i.e., a member of M, this is called *confirmation* (or corroboration) of UT.

In the present set-up Popper's distinction between universal and existential statements is thus interpreted as the distinction between the general claim of the theory ($I \subseteq M$) that all intended applications are models of the theory, and the negation of this claim, the existential claim that at least one intended application is not a model (I-M is non-empty).

A *basic statement* becomes a claim to the effect that a certain intended application x in I belongs to a certain subset F of Mp, defined by a certain condition on potential models, i.e., $x \in I \cap F$. An *accepted* basic statement presupposes of course that the relevant intended application has been realized.

The basic statement $x \in I \cap F$ is in conflict with theory *UT* if it can be demonstrated on conceptual grounds that $F \cap M$ is empty. Such basic statements may be seen as more direct explication of Popper's idea of 'potential falsifiers', compared to 'potential counter-examples'. However, it is easy to show that the suggested statement concept of potential falsifier has become essentially redundant. It is easy to check that a true potential falsifier of *UT*, i.e., "$x \in I \cap F$" is true, implies that *I-M* is non-empty and hence that x is a counter-example of *UT*. Conversely, the existence of counter-examples of *UT* is easily seen to imply that there must be true potential falsifiers. By consequence, demonstrating empirically the existence of a counter-example, i.e., realizing a potential counter-example, goes hand in hand with demonstrating that there is a true potential falsifier. Hence, the statement concept of potential falsifier is not needed in the face of the counter-example concept.

The Slide-Balance Reconsidered

The problem with the slide-balance is that it might be impossible to test the claim in a non-circular way without leading to infinite regress. For, to test the claim it is necessary to measure the distances and the weights. Whereas distance measuring does not require something like a slide-balance, weight measuring may not only actually be done by using a slide-balance, there might even be no other possibility. If the weight of a particle is measured by a slide-balance the law of the balance is obviously presupposed. Hence, assuming that the weight of a particle can only be measured by a slide-balance, the concept of weight is *SB*-theoretical and leads to the so-called *problem of theoretical terms*. A non-circular test of the claim of the theory would presuppose that the weights of the particles have been measured before with the same or another slide-balance. Hence, we get either an infinite regress or circular testing, if we stick to "$I \subseteq M$" as the empirical claim of the theory. There is, however, a way out of this dilemma, by restricting the empirical claim to *SB*-non-theoretical terms. Later we will see that the situation in the present example is for two reasons not as dramatic as suggested, but this did not exclude that the example could, by way of thought experiment, be transformed into an instructive example of genuine theoretical terms.

In order to formulate a new empirical claim we introduce the set of potential partial equilibrium models *SBpp*, being the structures of *SBp* without the *SB*-theoretical weight component and the corresponding 'status-condition', viz. clause 3). By consequence, there is a restriction or *projection function* π from *SBp* onto *SBpp* projecting every potential model on the potential partial model arising from deleting w and clause 3). Hence, for $x = <P,Pl,d,w> \in SBp$ the projection of x, $\pi(x)$, is equal to $<P,Pl,d> \in SBpp$. For an arbitrary subset X of *SBp*, πX, the projection of X, is defined as the subset of *SBpp* containing precisely the projections of the members of X.

For stratified theories we assume that the set of intended applications E no longer represents the equilibrium states seen through *SBp*, but seen through *SBpp*. Hence, the corresponding conceptual claim that E is a subset of *SBpp* is not laden with our theory about *SB*.

Now it is easy to see that the claim that E is a subset of πSB is not laden with the weight term, in the sense that it does not presuppose that the weights of the particles have been empirically determined. Hence, this revised empirical claim can be tested in a non-circular way.

It is plausible to call the members of $E\text{-}\pi SB$, if any, counter-examples of the theory. It is clear that they have to come from $SBpp\text{-}\pi SB$. Hence, it is now plausible to call this set the empirical content and its members potential counter-examples. Note that the empirical content reduces to the empirical content of the unstratified theory (*SBp-SB*) when *SBpp* and *SBp* are identical and π is, by consequence, the identity-function.

Unfortunately, the new claim is not only non-circular, it is also vacuous, for the empirical content is empty. The claim says in fact that all intended applications can be extended to models of the theory. To be precise, the claim is that every $<P,Pl,d>$ in E can be supplied with a positive real-valued function w on P such that $<P,Pl,d,w>$ is in *SB*. But it is easy to check that this is possible for every member of *SBpp*. In other words the empirical content $SBpp\text{-}\pi SB$ is empty.

However, the situation changes when we take so-called constraints into consideration: in the present case we have to require also that the weights assigned to the same particle, occurring in different applications, should be the same. In contrast to the distance of the objects from the fulcrum, our concept of weight is such that the weight of particles is constant in different applications. The formal treatment of constraints, however, will be postponed to a later section.

Before I return to the general exposition I will summarize the formal features of the theory of the slide-balance, leaving out the plausible specification of the projection function π:

The refined theory of the slide-balance $< SBp, SBpp, SB, \pi, D, E >$

$\vert\longrightarrow$	contains $< P, Pl, d, w >$	iff
$\vert\rightarrow$	contains $< P, Pl, d > = \pi < P, Pl, d, w >$	iff
	1) P is a finite set and Pl a subset	(particles) (the particles on the left of S)
SBpp	2) d: $P \rightarrow \mathbb{R}^+$	($d(p)$: the distance of p from S)
SBp	3) w: $P \rightarrow \mathbb{R}^+$	($w(p)$: the weight of p)
SB	4) *the law of the balance* $\Sigma p \in Pl\ d(p) \cdot w(p) = \Sigma p \in P\text{-}Pl\ d(p) \cdot w(p)$	
$SBpp\text{-}\pi SB$	*empirical content* (without w-constraint empty, with w-constraint non-empty)	
$E \subseteq SBpp$	*conceptual claim*: the domain of intended applications D can be represented as potential partial models	
$E \subseteq \pi SB$	*empirical claim*: the intended applications can be extended to models (strong claim: $E = \pi SB$)	

By way of digression, it is interesting to note that, assuming the weight-constraint, the *SB*-theory explains the following experimental, i.e., *SB*-unladen, *factor slide law*: if, starting from an equilibrium, the distances of all objects are multiplied by the same factor, there is again equilibrium. For it follows trivially from the law of the balance that it remains satisfied.

As a matter of fact, in the present case it is not difficult to formulate an experimental law such that the notion of weight can be explicitly defined, apart from a proportionality constant, on its basis. The law referred to states the following: given a unit object at a unit distance at one side of S, every other object p has a 'unique equilibrium distance' $d_u(p)$ at the other side. The weight $w(p)$ is then defined as $1/d_u(p)$, hence, such that in the relevant cases the law of the balance is satisfied by definition. Hence, for these cases the law cannot be tested in a non-circular way. But there is no regress, let alone infinite regress. For, given the definition, the rest of the law of the balance is a straightforward empirical claim which can be directly tested.

By consequence, on closer inspection the theory of the slide balance does not give rise to the problem of theoretical terms, when certain experimental laws are taken into consideration. Of course, this does not affect the instructiveness of the *SB*-theory as an almost proper theory. Moreover, it illustrates an interesting way in which a seemingly proper theory may be on closer inspection a sophisticatedly formulated experimental theory, in the present case: the conjunction of the 'unique equilibrium distance law', the weight-definition on its basis, and the law of the balance.

There is still one other reason why the problem of theoretical terms is not as dramatic in the case of the slide-balance: there are other ways of measuring the weight of objects than by using a slide-balance. But let us now turn to the general set-up of stratified theories, designed for proper theories.

Stratified Theories

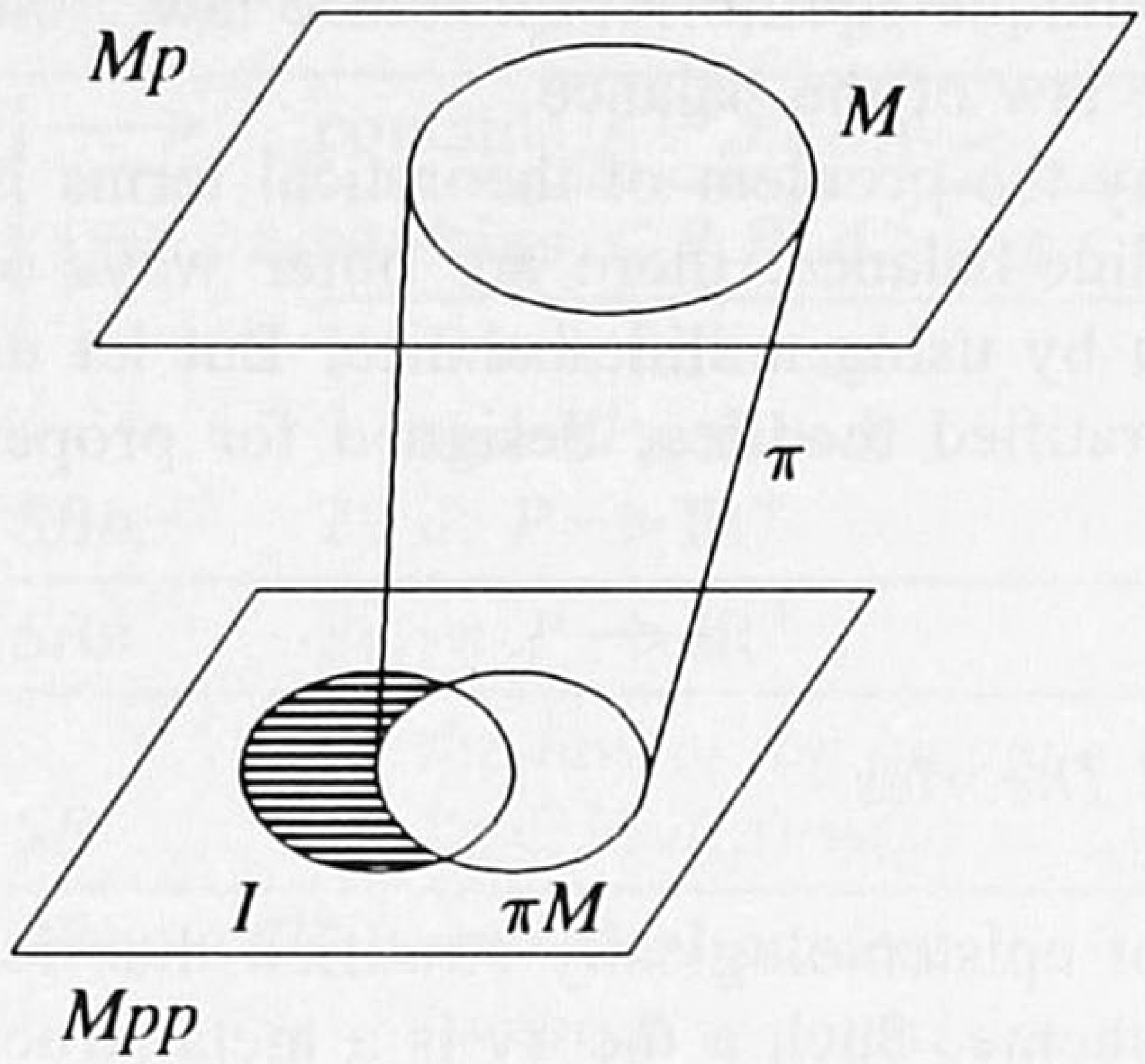
The general set-up of the structure of epistemologically stratified theories can now directly be presented in a scheme. Such a theory is a meta-structure of the following form:

	$<Mp, Mpp, M, \pi, D, I>$ is an *epistemologically stratified theory* iff
Mp:	potential models: a set of structures of a certain type
Mpp:	potential partial models: the substructures of Mp restricted to non-theoretical components
$M \subseteq Mp$:	models: the potential models that satisfy all axioms
$\pi: Mp \rightarrow Mpp$:	the projection function (from Mp onto Mpp) $\pi X = \{\pi(x)/x \in X\}$, for $X \subseteq Mp$, implying $\pi X \subseteq Mpp$
πM:	projected models
$Mpp\text{-}\pi M$:	empirical content
D:	intended domain of applications
$I \subseteq Mpp$:	intended applications (non-theoretical): conceptual claim: non-theoretical representation of D leads to the subset I of Mpp
$I \subseteq \pi M$:	empirical claim (strong claim: $I = \pi M$)

Examples

In the literature there have been presented structuralist reconstructions of many well-known theories. Here is a short list with references:

Newton's classical (gravitational) particle mechanics: Sneed [1971], Zandvoort [1982] and Balzer, Moulines, Sneed [1987]. In this example the distinction between general and special principles of a theory is fundamental.



Now it is plausible to call $<Mp, Mpp, M, \pi>$ the *theoretical core* of the theory and $<D,I>$ remains the *application target*.

Figure 2 illustrates the revised empirical claim: the shaded area, representing $I\text{-}\pi M$, should be empty (on conceptual grounds!).

Figure 2

Simple equilibrium thermodynamics [Balzer et al., 1987]
Mendeleev's and the refined theory of the periodic table of chemical elements: Hettema and Kuipers [1988].
Freud's psycho-analytic theory: Balzer [1982] and Stegmüller [1986]. In this example the distinction between absolute and relative empirical content [see below] is very important.
Arrow-Debreu's neoclassical theory of individual and collective demand: Janssen & Kuipers [1989]. In this example there are other than epistemological reasons for the distinction between potential models and partial potential models.
Jeffrey's theory of decisions [Stegmüller, 1986]
Jakobson's theory of literature [Stegmüller, 1986]

In the Balzer [1982], Diederich [1981], Stegmüller [1973/1986] and Balzer, Moulines, Sneed [1987] one can find numerous other examples.

From the fact that the theories of Freud and Jakobson can be reconstructed in the structuralistic way it follows that this way of reconstruction is, like the statement approach, applicable to qualitative, non-mathematical theories. From the other examples it is evident that the present approach is also well suited for quantitative theories, a kind of theory for which the statement approach leads to all kinds of complications.

In a sense it is a trivial claim that every empirical theory can be reconstrued in structuralist fashion. Hence, there should be additional reasons to do so in particular cases. A general reason frequently is the desire to get a better insight in the theory; beside that, one may be interested in particular questions, such as whether the theory has empirical content, whether it is an experimental or a proper theory, what its precise relation is to another theory, etc. But the main function of getting acquainted with the structuralist approach in general, and by examples, is of course the heuristic role it may play in the construction of new theories.

Absolute and Relative Empirical Content

It is always possible to divide the axioms on the one hand into analytic (*A*) and substantial (*S*) axioms and on the other into non-theoretical (*N*) and theoretical (*T*) axioms, leading to four types of axioms: *NA*, *TA*, *NS*, *TS*. For both distinctions it holds that it is advisable in case of doubt to choose the cautious classifications, i.e., *S* and *T*, respectively.

The following survey will speak for itself.

Mpp	(the set of potential partial models):	*NA*
Mp	(the set of potential models):	*NA* + *TA*
Mpart	(the set of partial models):	*NA* + *NS*
M	(the set of models):	*NA* + *TA* + *NS* + *TS*

It is clear that <*Mpp,Mpart*> is the (theoretical) core of a partial theory, i.e., an unstratified, hence experimental theory, constituting a substantial part of the full theory. The empirical content of the full theory was defined as *Mpp*-πM, let us call it more specifically the *absolute empirical content* (AEC). The empirical content of the partial theory, the *partial empirical content* (PEC), is of course *Mpp-Mpart*. Given the trivial fact that πM is a subset of *Mpart*, the partial empirical content is automatically a subset of the absolute empirical content. The interesting question is whether the full theory has something to add to the partial theory, i.e., whether the *relative empirical content* (REC), defined as *Mpart*-πM, is non-empty.

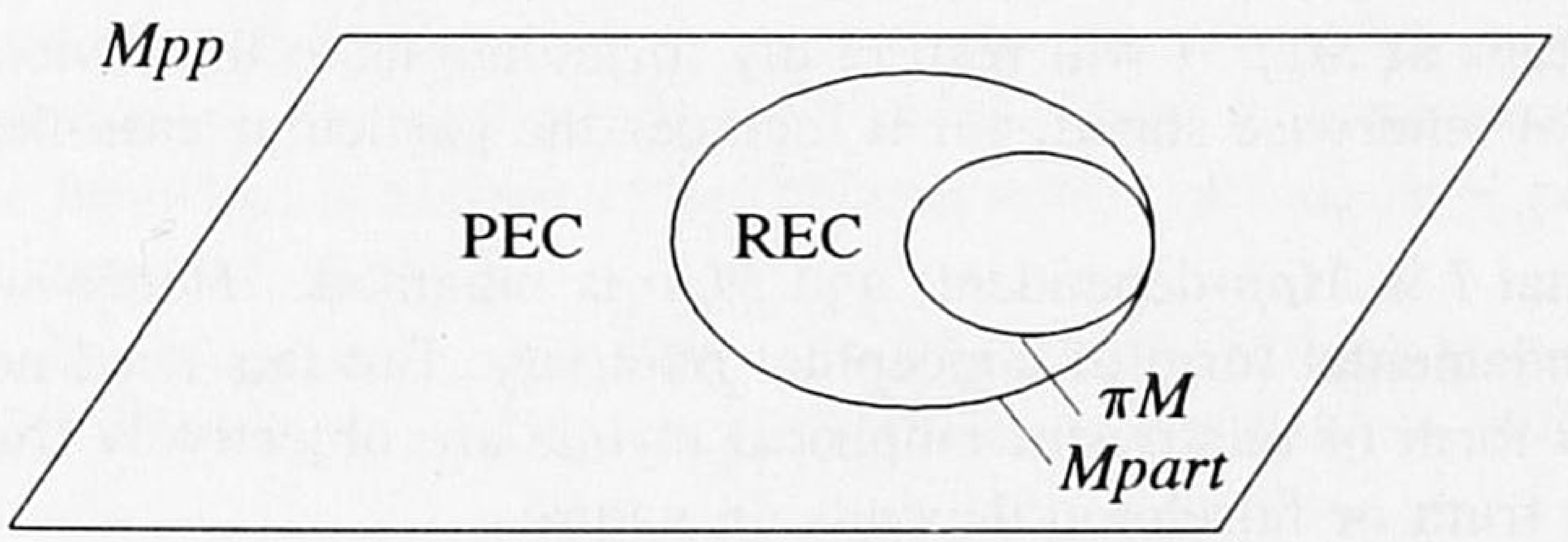


Figure 3

It is easy to check that the absolute empirical content (*Mpp*-πM) is the union of the partial empirical content (*Mpp-Mpart*) and the relative empirical content (*Mpart*-πM). *Figure 3* depicts PEC and REC explicitly. AEC is the union of both.

As a consequence, if a theory has relative (and/or partial) empirical content it has absolute empirical content. Conversely, however, a theory may have absolute empirical content without having relative empirical content, in which case the absolute empirical content coincides with the partial empirical content.

Balzer [1982] claimed that the general psycho-analytic theory has partial, and hence absolute, but no relative empirical content. Stegmüller [1986], however, is able to prove that it also has relative empirical content. Stegmüller then continues with the interesting observations that for this proof it is not necessary to take constraints and/or special laws into consideration and that classical particle mechanics CPM has only (relative) empirical content when constraints and special laws are taken into considerations. Hence, according to the relative content criterion for empirical impact the theory of Freud is in a sense even superior to that of Newton.

But we like to add that the CPM example makes clear that a generic theory (including constraints) need not have relative empirical content in order to be useful. The important research question is whether a generic theory can be supplemented with special laws which have relative empirical content [Cf. Bunge, 1977].

Intended Applications Reconsidered

In this section I will start with some general remarks about the set of intended applications *I*, then I will formulate three different ways of determining *I*, and conclude with some elaboration of the problem of theoretical terms.

I was introduced as '*D* seen through *Mp*' and later revised as '*D* seen through *Mpp*', i.e., *I* represents the intended domain of applications with the conceptual means of *Mpp*. I will restrict my formulations to the revised situation, when not otherwise stated, for it includes the particular case that *Mp*=*Mpp*.

It is evident that *I* is *Mpp*-dependent, and *Mpp* is manmade. Hence we subscribe to a fundamental form of conceptual relativity. But this need not imply an extreme form of relativism: empirical claims are objectively true or false, for their truth or falsehood depends on nature.

In its turn, the objective character of empirical claims does not imply that *D*, *Mpp* (and hence *I*) and *Mp* are fixed beforehand, and that the task remains to formulate a subset *M* of *Mp* leading to a true empirical claim. As a matter of fact, in practice, the determination of *D*, *Mpp*, *Mp* and *M* is a complicated dialectical interaction process, guided by the desire to formulate informative and true empirical claims. Unfortunately, it seems difficult to discern general patterns in this interaction process, without making some important idealizations.

However, if we assume, by idealization, that *Mpp* is fixed, the determination of (*D* and hence) *I* can be governed by at least three different principles.

If we are interested in all relevant empirical possibilities, *I* coincides with the set of empirical possibilities at the *Mpp*-level. Let *EMPOSpart* indicate this subset of *Mpp*. Although we may not have an explicit characterization of *EMPOSpart*, there is a clear empirical criterion for membership: *x* in *Mpp* belongs to I=*EMPOSpart* iff *x* can be empirically realized. If we are interested in a well-defined subset of the set of *EMPOSpart*, i.e., empirical possibilities satisfying some explicit condition, membership determination is not fundamentally different. In both cases we will speak of *empirical determination* of *I*.

In this case, the obvious target of theory development is an explicit charaterization of *I*, i.e., a set of models *M* is sought for which the strong claim holds: $I=\pi M$, such that *M* may be called *the true (Mp-)theory about I*, or simply, *the (conceptually relative) truth*. In Kuipers [1992a/b] the formal structure of truth approximation by unstratified and stratified theories is studied (extensively/briefly).

The notion of an empirical possibility is here presented as an absolute qualification, but there may well be cases where it makes good sense to distinguish levels of empirical possibility, as e.g. suggested by the following sequence of 'lower' to 'higher' levels: the physical, chemical, biological, psychological, cultural-socio-economical level. Being empirically possible at a higher level then implies being empirically possible at a lower level, but not the converse. Another example concerns the idea of empirically possible states of an artifact, assuming that it remains intact, which means a severe restriction to its physically possible, including broken, states. Such refinements can easily be built into empirical determination of *I*, as long as the boundaries between the different levels of empirical possibility may be assumed to be sharp.

In many cases, however, the interest is directed to a proper subset of *EMPOSpart*, of which the membership is not sharply defined. One important way in which I can then have been circumscribed is by so-called *paradigmatic determination*.

Definition: I is *paradigmatically determined* if there are PAR and SIM such that

1. I is a subset of $EMPOSpart$	the intended applications
2. PAR is a finite subset of I	the paradigmatic examples
3. SIM is a binary relation on Mpp	a similarity relation
4. for all x in I–PAR there is y in PAR such that $SIM(x,y)$	

The elements of PAR may for instance be determined by the founding father of the theory and corresponds to one of the meanings Kuhn [1962] had in mind with the term 'paradigms' and which he later called 'exemplars'. Of course, the main source of vagueness is the notion of similarity, for it will as a rule not be possible to define this notion sharply, at least not at the beginning of the research process.

In both cases of determination, assuming that the theory has (at least absolute) empirical content, the empirical claim of a stratified theory will not be trivial. As is easy to verify, the empirical claim becomes trivial in the third way of determination of I: so-called *auto-determination*: for x in $EMPOSpart$, x belongs to I iff x belongs to πM, i.e., x is the projection of a model of a stratified theory. In the case of auto-determination the theory in question is typically not something to be tested, but it will have been designed for other purposes.

In case of an unstratified theory the set of intended applications is of course a subset of the set of empirical possibilities at the Mp-level, which is then on its turn a subset of Mp. For the further determination of I there are again the same three possibilities of empirical, paradigmatic and auto-determination. In the case that there are proper theoretical terms involved, all three forms of determination lead to problems.

Let us briefly restate and elaborate the background of (epistemological) stratification of a theory in T-theoretical and T-non-theoretical terms. Let there be an unstratified theory $UT = \langle Mp, M, D, I \rangle$ and assume that UT has non-empty empirical content Mp-M. The term t occurring as component in Mp is said to be T-theoretical iff every known method of measuring of t in a specific intended application results in a model of UT. It is T-non-theoretical otherwise. Let UT contain at least one T-theoretical term and let us first assume that I is supposed to be empirically or paradigmatically determined, in which case the empirical claim "I is a subset of M" is non-trivial. However, testing this claim is impossible, for it leads either to circularity or to an infinite regress, as is not difficult to check. In the case of auto-determination the problem is that determination of the membership of M leads to circularity or infinite regress.

The remedy for these problems is the epistemological stratification of the theory in terms of a partial theory containing precisely all T-non-theoretical terms. Assuming that the stratified theory has non-empty (absolute) empirical content $Mpp\text{-}\pi M$, either the empirical claim or auto-determination is non-trivial, depending on whether I has or has not been fixed in advance, respectively.

The indicated definition of T-theoreticity is a pragmatic one, due to the "every known method of measuring"-clause and goes back to Sneed [1971]. Given the fact that the class of known methods can only increase, the definition is perfectly compatible with the advice to classify a term as T-theoretical in case of doubt. However, it is tempting to look for an intrinsic definition of theoreticity. Gähde [1983] has put forward an intrinsic definition. However, this proposal is not only highly technical and restricted to quantitative terms, it has also been criticised severely [Cf. Schurz, 1990].

Idealization and Concretization

Theories which are roughly about the same domain are frequently related. At each moment they may constitute a network of theories, i.e., a partially ordered set of theories that are directly or indirectly related. Such a network depicts the synchronic situation, the succession of networks indicates the diachronic development. The main relations studied in the structuralist approach are those of *specialization*, *theoretization* and *reduction*. For the purpose of this contribution, however, a fourth relation is more important: the relation of *concretization*, or its converse *idealization*.

In this section the relation of concretization will be defined and illustrated by the transition of the theory of ideal gases to that of Van der Waals. Some remarks will be made about concretization as a truth approximation strategy.

Concretization or factualization, as it has been presented by the Polish philosophers Wladislaw Krajewski [1977] and Leszek Nowak [1980], is basically a relation between real-valued functions. Hence, let us assume that the structures to be considered contain one or more base-sets and one or more real-valued functions defined on them, with or without one or more real constants. Structure y is called a *concretization* of x and x an *idealization* of y, indicated by $con(x,y)$, if y transforms, directly or by a limit procedure, into x when one or more constants or functions occurring in y uniformly assume the value 0. It is easy to see that it is a necessary condition for *con*(x,y) that x and y have the same base-sets. Moreover it is easy to check that *con* is reflexive, antisymmetric and transitive.

The next task is to define the binary relation of concretization between theories. We will do this as weakly as possible: Y is a *concretization* of X and X an *idealization* of Y, indicated by $CON(X,Y)$, if and only if all members of X have a concretization in Y and all members of Y have an idealization in X. At first sight one might think that the second clause should be strengthened to: and all members of Y have a *unique* idealization in X. However, this would exclude e.g. 'inclusive' concretization triples $<X,Y,Z>$ with X as subset of Y and Y of Z and $CON(X,Y)$ and $CON(Y,Z)$.

It is trivial that CON is reflexive and transitive. However, it need not be antisymmetric, contrary to what one might expect. But sufficient for antisymmetry of $CON(X,Y)$ is that X and Y are convex. X is *convex* is defined as: for all x and z in X, if *con*(x,y) and *con*(y,z) then y is in X.

The transition from the theory of ideal gases to Van der Waals's theory of gases has frequently been presented as a paradigmatic case of concretization. So let us start by formulating the relevant models in naive structuralist terms, without the (theory-relative) distinction between theoretical and non-theoretical terms. $<S,n,P,V,T>$ belongs to the set of *potential gas models* (*GMp*) iff S represents a set of thermal states of n moles of a gas and P, V and T are real-valued functions defined on S representing pressure, volume and (empirical absolute) temperature, respectively.

Specific gas models are *GMp*'s satisfying an additional condition. *Ideal gas models* (IGM) satisfy in addition $P(s)V(s)=nRT(s)$ for all s in S, or simply $PV=nRT$, where R is the so-called ideal gas constant. For *Van der Waals gas models* (WGM) there are non-negative real constants a and b, within certain fixed intervals, such that $(P+(n^2a/V^2))\ (V\text{-}nb)=nRT$.

Note first that it is a necessary condition for *con*(x,y) (x and y in *GMp*) that $S_x=S_y$. Note also that IGM and WGM are convex.

It is easy to check that WGM is a concretization of IGM (formally: *CON*(IGM,WGM)): each element of WGM transforms into an element of IGM by substituting the value 0 for a and b, and with each element of IGM there even can be associated several elements of WGM corresponding to arbitrary selections of values for the constants a and b within their respective intervals.

In Kuipers [1992a/1992b] I show among others that my refined definition of truthlikeness is such that "truth approximation by concretization" is perfectly possible. I illustrate this by indicating that WGM is closer to the truth than IGM, assuming the heuristic hypothesis that the true set of empirically possible gases is, in its turn, a concretization of WGM.

In these articles I also point out how concretization can also play a crucial role in not directly empirical scientific research directed at proving interesting theorems for certain sets of structures, as Hamminga [1983] showed for neo-classical economics. In another paper this type of concretization aiming at provable interesting truths is illustrated by the concretization, due to Kraus and Litzenberg, of the theory of Modigliani and Miller concerning the capital structure of firms [Cools, Hamminga and Kuipers, to appear].

Constraints

I have referred several times to so-called constraints. Whereas laws and axioms in the normal sense lay down restrictions to individual potential models, constraints impose restrictions to sets of potential models. A particular type of constraint is a so-called identity-constraint, guaranteeing that a function assigns in different potential models, with some common base-sets, the same value to the same individual. The weight-function in the case of the slide-balance as well as the mass-function in the case of classical particle mechanics are cases in point.

A constraint can be formally defined in a very general way.

Definition: C is a *constraint* on the set S iff
1) C is a set of subsets of S
2) the union of the sets in C exhausts S ($\cup C=S$)

C is a *transitive* constraint if it satisfies in addition:
3) if X is in C and Y is a subset of X then Y is in C (subset-preservation, or transitivity)

Although there are interesting cases of non-transitive constraints the following remarks presuppose transitivity. To begin with, it is easy to prove that all singleton sets $\{x\}$, for x in S, belong to C, hence a (transitive) constraint does not exclude any individual potential model.

Let us now first concentrate on the typical role of a constraint C on Mp in a stratified theory $ST=<Mp,Mpp,M,C,\pi,D,I>$. The standard empirical claim was "I is subset of πM", which could be paraphrased by saying that all members of I can be extended with theoretical components to genuine models, i.e., there is a subset X of M such that $\pi X=I$. Taking the constraint into consideration this claim is strengthened to: there is a subset X of M *belonging to* C such that $\pi X=I$. Hence, now both M and C restrict the degrees of freedom for the supplementation of theoretical components. In the corresponding versions of the strong claim the clause "X is a subset of M" is simply replaced by "$X=M$". It is clear that a stratified theory may even be a pure *constraint-theory*, in the sense that C is non-trivial and M is trivial, i.e., $M=Mp$.

The following reformulation of the standard claim with a constraint is instructive. Let $A(ST)$, the *application space* of ST, be defined as the set of projections of all subsets of M satisfying C (formally: $A(ST)=\pi(P(M)\cap C)$). The standard claim comes now down to: I is in $A(ST)$.

It is also plausible to define now the *(absolute) empirical content* $AEC(ST)$ as $P(Mpp)$-$A(ST)$, i.e., the subsets of Mpp which are excluded by M and C. Note that $AEC(ST)$ reduces to $P(Mpp)$-$\pi(P(M))$ when C is trivial, i.e., when $C=P(M)$, and to $P(Mp)$-$P(M)$ when π is the identity function. It is easy to check that $AEC(ST)$ is empty in these respective cases iff the originally defined empirical contents Mpp-πM, respectively, Mp-M are empty. Hence, the suggested new definitions of empirical content reproduce the original ones on the level of sets of sets of potential (partial) models.

Similar relations hold for the plausible definition of the *relative empirical content* $REC(ST)$: $P(Mpart)$-$A(ST)$. And again it follows almost trivially that non-empty $REC(ST)$ implies non-empty $AEC(ST)$, but not the converse.

Constraints make also sense in other cases, e.g. in partial theories and in unstratified theories. For the first case, let $Cpart$ be a constraint on Mpp. Like $Mpart$, $Cpart$ represents empirical restrictions. We may say that $Mpart$ captures the standard empirical laws, whereas $Cpart$ captures *constraint empirical laws*. Of course, the associated claim states that I is a subset of $Mpart$ belonging to $Cpart$. It is important to note that many empirical laws are constraint laws, or mixtures of standard and constraint laws.

If the indicated partial theory with constraint is isolated from the full theory, it is clear that we have an unstratified theory with constraint. In general, an unstratified theory with constraint is of course of the form $<Mp,M,C,D,I>$, where C is a constraint on Mp.

Non-empirical Theories

Following Popper, non-empirical theories are by definition theories which are not (intended to be) falsifiable. One may distinguish at least four types of non-empirical theories:

metaphysical theories are supposed to make claims about reality without assuming any particular conceptualization or, equivalently, they make claims generalizing over conceivable conceptualizations of reality,

mathematical and logical theories deal with defined abstract objects, i.e., mental constructs,
conceptual theories concern ways of looking (perspectives) to a certain domain,
normative theories deal with what is (supposed to be) ethically, juridically, esthetically (in)admissable.

It is evident that almost all technical ingredients presented for empirical theories are also useful for non-empirical theories. In fact, Suppes [1957] invented the structuralist representation of empirical theories by transferring, as far as possible, the standard way of presenting mathematical theories to empirical theories. The crucial difference is that non-empirical theories do not make general empirical claims. The claims which are associated or made by them typically are either conceptual (logical, mathematical etc.) or restricted to individual intended applications. A typical claim in a mathematical theory is a mathematical theorem to the effect that the models of the theory can be proven to have a certain explicitly defined property. A typical claim of a specific conceptual theory is that a certain intended application is (or is not) a model of that special theory. Of course, generic theories, i.e., theories with vacuous empirical claim, are conceptual theories.

The 'structuralist theory of the structures of empirical theories' is a perfect example of a theory which is primarily intended as a conceptual theory (although one may strengthen it to a genuine empirical theory). As a consequence, the foregoing exposition provides not only an elaborated example of a conceptual theory, it can also convince the reader of the usefulness of conceptual theories.

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